XXIII. *A Second Paper on Hygrometry.*

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PART II.

IN the firſt *part* of this Paper * I have treated of the fundamental principles of hygrometry, and of ſome hygroſcopic phænomena; and this will relate to a particular application of thoſe premiſes.

62. Since the publication of my firſt hygrometer, many others have been invented, two of which are now principally in uſe; the *hair* hygrometer of M. DE SAUSSURE, and my *hygrometer* made of a *ſlip of whalebone*. If the comparative points of thoſe inſtruments could be determined in the whole extent of their ſcales, the only inconvenience of their being both uſed would be, the neceſſity of reducing to one of them, the obſervations made with the other; but from 70 to 100 of mine, which ſpace includes the moſt important period of *moiſture*, their correſpondent indications are as different from one another, and as variable, as if they were the effects of two very different cauſes. Therefore it is important to decide which of them ſhould remain our only meaſure of *moiſture*, till, if poſſible, a better one is found. The following pages, I hope, will lead to that deciſion.

* See Page 1. of this Volume.

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63. The fundamental proceſs of M. DE SAUSSURE, with the view of diſcovering the effects of *moiſture* on the *hair hygrometer*, was this. He repeatedly cauſed ſucceſſive known quantities of *water* to evaporate into a cloſe glaſs veſſel, previouſly reduced to *extreme dryneſs*, and containing that *hygrometer* and a *manometer*; he obſerved the correſpondent changes of thoſe inſtruments, and, by combining the reſults of his experiments, he reduced to regular *ſeries* the correſpondent motions of the two inſtruments by equal quantities of *evaporated water*. Having confined himſelf to that only claſs of experiments, which, from cauſes that I ſhall explain, could not diſcover to him the difficulties of his attempt, he thought himſelf warranted to draw from them the following concluſions. 1ſt, That the degrees of *moiſture* in the incloſed *medium*, were nearly proportional to the quantities of *water* evaporated in the veſſel; and that, conſequently, the *ratio* obſerved between thoſe quantities and the *march* of his *hygrometer*, could be conſidered as giving immediately the *march* of the inſtrument correſpondent to *moiſture* itſelf; which, according to our common opinion, is a certain quantity of *aqueous vapours* ſpread in the *medium*. 2dly, That when no more *water* could *evaporate* in the veſſel, the incloſed *medium* was arrived at *extreme moiſture*; and that, conſequently, the point indicated at that time on his *hygrometer*, was to be the *limit* of its *ſcale* on that ſide. 3dly, That having, from thoſe experiments, a probable determination of the *expanſions* of the *hair* by ſucceſſive equal quantities of *moiſture*, in beginning from the point were this is null, and ending at its *extreme*, his inſtrument could not differ eſſentially from an abſolute *hygrometer*.

64. Theſe concluſions were very natural in the ſtate of M. DE SAUSSURE's experiments; but before their publication I had gone

gone over a great field of *hygroſcopic* phænomena, in which the *hair*, and a *cloſe veſſel*, had a ſhare; and thereby, ſeeing the objects in another light than M. DE SAUSSURE, I doubted of his concluſions, and I procured three of his *hygrometers*, in order to examine them on ſome particular points. It was after that immediate verification of my conjectures concerning his inſtrument, that I ſettled the following concluſions, very different from thoſe above. 1ſt, That MOISTURE, or the quantity of *vapour* ſpread in the *medium* itſelf, does not increaſe in an incloſed ſpace in proportion to the quantity of water evaporated in it; becauſe of an increaſing, but undetermined, part of that *water* being depoſited on the ſides of the veſſel; and that, conſequently, Mr. DE SAUSSURE's experiments could not afford the determination of a real *hygroſcopic-ſcale*. 2dly, That the circumſtance conſidered by him as a ſure ſign of *extreme moiſture* exiſting in the incloſed *medium*, namely, the *maximum* of *evaporation* in the ſpace, has only that effect when the *temperature* is very little above 32°; but that, by ſucceſſive increaſes of *heat* from that point, *moiſture* recedes farther and farther from its *extreme*; or from the point where no more *vapour* can be introduced in the *medium* without an immediate *precipitation*; though at the ſame time, there are ſucceſſive increaſes in the quantity of *vapour*, and thereby a conſtant *maximum* of *evaporation* correſpondent with the actual *temperature*. 3dly, That, in approaching to *extreme moiſture*, the *hair* hygrometer becomes *ſtationary*, and afterwards a little *retrograde*, in which *march* the unavoidable irregularities of every *hygroſcopic* ſubſtance produce frequent anomalies; from which cauſe it was very difficult for M. DE SAUSSURE, conſidering the form of his experiments, to diſcover the *hygroſcopic law* expreſſed by the ſecond concluſion; and with the unknown exiſtence of that

law,

law, to ſuſpect the *march* of his *hygrometer*: which accidental complication I ſhall explain hereafter.

65. When I publiſhed thoſe reſults of my experiments and obſervations, M. DE SAUSSURE rejected them; not from having made new experiments that had confirmed his opinions; but becauſe he conjectured inverſely, that my theory reſulted from a fallacious *march* of my *hygrometer*: and the well-earned reputation of that celebrated philoſopher engaged me to undertake every experiment that could help me to detect on which ſide was the error. I have related, in the firſt *part* of this Paper, ſome of thoſe experiments; and now, for their application, as well as for giving an account of ſome others, I ſhall follow more particularly M. DE SAUSSURE's proceſs.

66. In a large glaſs veſſel, containing (as I have mentioned above) a *manometer* and his *hair*-hygrometer, which veſſel he had previouſly reduced to a known ſmall diſtance from *extreme dryneſs*, M. DE SAUSSURE introduced from time to time a piece of *wet* cloth, which he weighed both before he put it into the veſſel, and when he took it out. The ſucceſſive increaſes in the quantity of *vapour* reſulting from that proceſs were indicated; on the *manometer*, by ſucceſſive increaſes in the quantity of the incloſed *elaſtic fluids*, which cauſed the quickſilver to aſcend more and more in that inſtrument; and on the *hygrometer*, by ſucceſſive expanſions of the *hair*. The *maximum* of *evaporation* was clearly indicated by the *manometer*; for, during every laſting *temperature*, the quickſilver, after having aſcended to a certain point, remained fixed at that point, notwithſtanding a longer ſtay of the *wet* cloth; and by repeating that operation at different *temperatures*, M. DE SAUSSURE determined the quantities of *evaporated water* that, in a given ſpace, and by a given *temperature*, produced the *maximum* of *evaporation*.

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That ſingle determination, from its great conſequences not yet generally attended to, would be ſufficient to fix the celebrity of its author, as I have already expreſſed many times in other works.

67. But with reſpect to a different concluſion from the ſame experiments, no leſs important to natural philoſophy, I have alſo ſaid, that M. DE SAUSSURE's *hygrometer* may have miſled him. We have ſeen, that the *manometer* indicated, by an indubitable ſymptom, the *maximum* of *evaporation*; for here the immediate cauſe is clearly known, namely, the *quantity* of *elaſtic fluids*; and it is evident, that the quickſilver muſt aſcend in the inſtrument in proportion to that *quantity*, and ſtop when it ceaſes to increaſe. But it is not the ſame as to the indications of the *hygrometer* in reſpect of *moiſture:* M. DE SAUSSURE found himſelf that they were far from proportional to the intenſities of their cauſe; and in the laſt ſtage of his experiments, though theſe indications did not vary much by the different *maxima* of *evaporation*, they however varied in the ſpace of 1 or 2 *degrees*. But as thoſe ſmall differences on the point where the *hygrometer* ſtopped in different experiments, did not follow apparently any law conformable to the *temperature*, M. DE SAUSSURE conſidered them as ſmall *anomalies*, unavoidable in hygroſcopic ſubſtances, and of little conſequence on a ſcale of 100 *degrees*; therefore, laying aſide that circumſtance, he could have no doubt, that, in every *temperature*, the *maximum* of *evaporation* in a cloſe ſpace was ſynonymous with the *maximum* of *moiſture* in that ſpace; while, from my experiments, theſe two ſuppoſed identical expreſſions may differ $\frac{1}{5}$, and ſometimes $\frac{1}{4}$, of the real ſcale of *moiſture*, which is the caſe in the *temperature* of only 75° or 80°.

68. Let us now ſuppoſe for a moment, that the above *hygroſcopic law*, and the *march* that I attribute to the *hair* hygrometer, are real. In that caſe, if, during a conſtant *maximum* of *evaporation*, the *temperature* varies from 32° to 80°, *moiſture* will diminiſh $\frac{1}{5}$ and even $\frac{1}{4}$ of the whole; or, in other words, the ſtate of the *medium* will be diſtant by ſo much from that in which a new introduction of *vapour* would be followed by a *precipitation*. But at the ſame time, in the whole of that period of *moiſture*, the *hair* hygrometer is ſuppoſed to move only 1 or 2 *degrees* backwards and forwards, with frequent irregularities. Therefore, in the hypotheſis, ſuch a great change of *moiſture* would be hardly ſuſpected from thoſe ſmall deviations of the *hair* hygrometer, in which at firſt nothing appears to be regular; and thereby it is evident, that, by confining himſelf to thoſe experiments, M. DE SAUSSURE could not diſcover thoſe two important laws of *hygrology* and *hygrometry*, of which I have here only ſuppoſed the exiſtence.

69. Let us ſuppoſe again, that the *hair* hygrometer had not exiſted before a certain number of other experiments; and that M. DE SAUSSURE, in his attempt to produce an inſtrument of that kind, had fixed on any of the *ſlips* made of *fibrous* vegetable or animal ſubſtances cut *acroſs* the *fibres*, of which many *hygroſcopes* had been made before in a coarſer manner; and that, in every other reſpect, he had proceeded as he has done with the *hair*. In that caſe, having placed his *hygrometer* and the *manometer* in the ſame veſſel, with a quantity of *water* ſufficient for producing the *maximum* of *evaporation* in every common *temperature*, and obſerved alſo the points where both inſtruments ſtopped in different laſting *temperatures*, he then would have found; that the *hygrometer* indicated leſs and leſs *moiſture*, at the ſame time that the *manometer*, by aſcending more

more and more as the *evaporation* increaſed by more *heat*, indicated that increaſe in the quantity of *vapour*, diſtinct from the expanſions of the *fluids*; and that each of thoſe inſtruments remained alſo fixed in that ſtate of oppoſite changes, by every ſufficiently durable change of *temperature*. The firſt obſervation of that phænomenon would have ſurprized him, as it did me, and it would have induced him alſo to try ſome other of thoſe *ſlips*; and by finding the ſame phænomenon with every one of them, he would have been convinced, that it was a real *law* of *moiſture*. Laſtly, if he had known Mr. JAMES WATT's obſervations on the great *dryneſs* of the *ſteam* of *boiling water*, as long as it remains in a ſpace as *warm* as the *water* that produces it (a condition always underſtood when the general laws of *evaporation* are the object of inquiry); admitting with me, as it follows from his own theory, that ſuch *ſteam* is no other than the ſame kind of *vapour* thus far mentioned, only rendered capable of a greater *denſity* by more *heat*; he would then have ſeen the importance of that *hygroſcopic law* from its great extent. I have hardly any doubt, that *extreme dryneſs* would reign in a cloſe place ſimilar to PAPIN's *digeſtor*, if there were a ſufficient ſpace above the *water* and a *red heat*; though that ſpace would be filled with *vapour* to the *maximum*. This relates only to the *hygroſcopic law* here in view, on which, in the ſuppoſed caſe, I do not think we could have had any controverſy.

70. Laſtly, let us ſuppoſe, that in order to try the effect of *moiſture* on the ſubſtance he had firſt choſen, or on ſome other of the ſame kind of ſubſtances, but taken *lengthwiſe*, M. DE SAUSSURE had happened to try firſt one of thoſe which, uſed in that manner, have a great *retrogradation*, as *gooſe-quill* and *deal*; and that, after having previouſly obſerved it in the

open air, he had incloſed it in the *moiſt veſſel*, at a time when the place where it ſtood before had the degree of *moiſture* correſponding with the *ſtationary* ſtate of that *hygroſcope*, he would then have obſerved a phænomenon as little expected as the former: for after that inſtrument had been incloſed in the *moiſt veſſel*, it would have moved, by that increaſe of *moiſture*, in the ſame direction as it had done in the open *dry* air when there was on the contrary an increaſe of *dryneſs*. Surpriſed no doubt at that phænomenon, M. DE SAUSSURE would have ſubmitted his new inſtrument to more experiments; he would alſo have tried other *threads*, in which he would have found the ſame ſort of *march*, only at various degrees; and if, in the courſe of thoſe trials, he had ſubmitted the *hair* to the ſame experiments, the ſmallneſs of its motions backwards and forwards, and their irregularities, would not have prevented him from diſcovering in it the ſame ſort of *march* as he had then been uſed to ſee in other *threads*; and thereby, he would have abandoned the whole tribe of *threads* as unfit for the *hygrometer*.

71. The whole of that ſuppoſed courſe of experiments with *ſlips* and *threads*, is that which I have followed from the time I had abandoned the conſtruction of my firſt *hygrometer*; which I did eſpecially with the view of being able to try many ſubſtances. Therefore my theory was formed in conſequence of the two above concluſions, which appear to me immediate, and ſuch as M. DE SAUSSURE could not have drawn differently, if he had followed the ſame neceſſary ſteps: and now I will prove, moreover, that if it had not been for *accidental* circumſtances in his own proceſs, the *hair* alone would have engaged him by degrees to undertake the ſame experiments.

72. M. DE SAUSSURE's firſt *hygrometers*, having their *index* at the top of the frame, could be plunged into *water*; and he tried

tried that method for fixing their point of *extreme moiſture*, as I had done for my firſt *hygrometers*. But in thoſe trials he obſerved, that while his inſtruments ſtood in *water*, their indications remained undetermined within a ſpace of four or five degrees; and attributing that irregularity to a *friction* of the *hair* with *water*, he thought it neceſſary to change my method, to that of placing the *inſtrument* in a *moiſt medium*, which he produced by means of a glaſs jar, *wet* on the inſide, and inverted over *water*. In this method, the ſituation of the *index* became indifferent; and, for ſome particular reaſon, he placed it at the bottom of his new *hygrometers*, which then could not be plunged into *water*. This laſt circumſtance was merely accidental; however, we ſhall ſee how much it has influenced his opinions in reſpect of *extreme moiſture*.

73. In the firſt account of his experiments, M. DE SAUSSURE did not enter into thoſe particulars; but they are in his anſwer to me: and when I there ſaw, that he gave as a reaſon for having *abandoned the immerſion in water*, that a *ſtrong adheſion* of the *hair* to that liquid impeded the freeneſs of its motions, I found it more natural to aſſign to the inſtrument itſelf the unſteadineſs of its *index* which he attributed to that cauſe. Indeed, in thoſe firſt *hygrometers*, one end of the *axis* paſſed through a hole for the purpoſe of carrying the *index* outwards, which was a cauſe of much friction: that *axis* beſides was loaded with the weight of pretty large pincers, holding the *hair*, and that weight was counterpoiſed on the other ſide. Laſtly, the connexion of the *hair* with the *index* was produced by a *ſilver lamina*, which, though very thin, oppoſed ſome reſiſtance in bending round the *axis*. Thoſe are defects that M. DE SAUSSURE corrected afterwards; but they exiſted in the inſtruments which he plunged into *water*, and a weight of

only 3 *grains* was not ſufficient to keep their *index* ſteady, either in *water* or any where; and this he obſerved himſelf.

74. Notwithſtanding that natural explanation of the unſteadineſs of M. DE SAUSSURE's firſt *hygrometer* when in *water*, I thought it neceſſary to try, in the ſame circumſtance, ſome well-conſtructed *hair hygrometer*; therefore I made two, ſimilar to the laſt of M. DE SAUSSURE's in every reſpect, except that of having their *index* at the top; and for the connection of the *hair* with the *axis*, I uſed a kind of *pincers*, not above half a *grain* in weight, with a hair-like bit of *hemp*, which I know does not alter in any ſenſible manner the march of the inſtrument. Now, theſe *hair* hygrometers, with their *weight* of only 3 *grains*, being put into *water*, follow in it their own laws, arrive and remain *fixed* at their once ſettled point, as well as any of my other hygrometers. This M. DE SAUSSURE would have alſo found, if in the improvement of his inſtrument the *axis* had remained at the top.

75. Before I explain the influence which that *accidental* circumſtance of the place of his *index* has had on his opinions with reſpect to *extreme moiſture*, I muſt mention another of the ſame kind which has contributed to the ſame effect. It is evident, both from theory and from M. DE SAUSSURE's own experiments related above (§ 66.), that a ſufficient quantity of *water* in any part of a cloſe veſſel is the only requiſite for producing in it the *maximum* of *evaporation*; but, with a view of accelerating that effect for the common purpoſe of fixing the point of *extreme moiſture* on his *hygrometer*, he preſcribes *wetting* the inſide of the veſſel, beſides inverting it over *water*. He did not (and indeed he could not) foreſee the conſequence of that alteration in his firſt proceſs; but in fact it was ſuch as to prevent him from diſcovering, even with time,

time, in thoſe operations, what he would have ſeen immediately if his laſt *hygrometers* could have been plunged into *water.*

76. After the improvement of his *hygrometers,* their *index* was no more ſubject to that unſteadineſs obſerved in the firſt; conſequently they remained *fixed* under the *moiſt veſſel*; but they did not *fix* at the ſame point every time; and it happened *accidentally,* from cauſes of irregularity in the *veſſel* itſelf, that in ſome of the caſes, when the *hair* was the *longeſt,* a *precipitation* of *water* happened (by ſome partial cooling) on ſome part of that veſſel; and he took that appearance for a ſure ſign that there was a *ſuperfluous* quantity of *water* in the incloſed air. From that *accidental* connexion of circumſtances he concluded, that his *hygrometer* indicated two different ſtates of the *medium* in reſpect of *extreme moiſture*; one, correſpondent to about 98 on that inſtrument, which he conſidered as real *extreme moiſture,* or that ſtate of the *medium* in which no more *vapour* could be introduced into it without a *precipitation*; the other, when ſuch a *precipitation* took place, which he made correſpondent to 100, or to the greateſt *length* of the *hair.* Prepoſſeſſed afterwards with that opinion, when I publiſhed my comparative experiments of his *hygrometer* and mine, in which the latter ſtood ſometimes at 80 when the former was at 98, he concluded from that circumſtance, that while his *hygrometer* moved only 2 *degrees* by the utmoſt effect of a *ſuperſaturation* of the *medium,* mine had 20 of thoſe *inſignificant degrees.* In this centers the whole of our diſagreement, and I am now going to trace its cauſe in the *accidental* circumſtances above deſcribed.

77. When I plunge my *hair* hygrometers into *water,* where, as I have ſaid, they come to a *fixed* point, that point does not indicate the *greateſt length* of the *hair*; for, on the contrary, that

that *thread* is then *ſhorter* than it is moſt times under the *moiſt veſſel*. This M. DE SAUSSURE would have ſeen, if he had not been prevented, by the ſituation of the *index* in his improved *hygrometer*, from trying again the effect of *water* on the *hair*; and that phænomenon alone would ſurely have given a different courſe to his ideas; eſpecially he would not have ſuppoſed, that the *hair* lengthens 2 *degrees* more, by a *ſuper-ſaturation* of the *medium*, or by the immediate contact of concrete *water*.

78. If, alſo, when he ſettled the manner of determining the point of *extreme moiſture* on his *hygrometers*, M. DE SAUSSURE had retained the ſimplicity of the proceſs he had uſed for his fundamental experiments, in which a piece of *wet* cloth had been ſufficient for producing the *maximum* of *evaporation* in his large veſſel; and, in conſequence, had contented himſelf with inverting his glaſs jar over *water*, without *wetting* it on the inſide, he would have avoided a great cauſe of deception which I am going to explain. In my firſt experiments on the comparative *marches* of our *hygrometers*, in which I followed M. DE SAUSSURE's preſcription for the *moiſt veſſel*, I found ſome *anomalies* which puzzled me. M. DE SAUSSURE himſelf took notice of them in the account I gave of thoſe experiments, and attributed them to my inſtrument. I did not agree with him in that reſpect; but it was long before I could diſcover the real cauſe of thoſe anomalies. The firſt ſtep towards that diſcovery, was the reflecting on the uſeleſſneſs of *wetting* the veſſel on the inſide, for the only purpoſe of producing in it the *maximum* of *evaporation*. That conſideration engaged me to undertake a new courſe of the ſame experiments, with a glaſs jar merely inverted over *water*; and by that means, the greateſt part of the real *anomalies* being removed, I obſerved clearly in the *march* of the *hair*, the

combined effects of its own property, and of the *hygroſcopic law* reſpecting *evaporation* which was known to me from other phænomena.

79. As for the cauſe of thoſe *anomalies* which had been removed by the change of the proceſs, the following phænomenon led me to diſcover it. I obſerved frequently, at times when my *hygrometer*, placed under the jar, ſtood at a conſiderable diſtance from its point of *extreme moiſture*, that a very ſmall diminution of heat was ſufficient to cauſe, on the lower part of the veſſel, the formation of a *tarniſhed* rim, extending one or two inches above the ſurface of the *water*, with a thin vaniſhing edge. Having reflected on that phænomenon, from the mechaniſm I aſſign to the operation of *fire* in the very act of *evaporation* I concluded, that in ſuch a *ſtagnant air* every *evaporating* ſurface had an *atmoſphere* of *extreme moiſture*, which extended as indicated by the *tarniſhed rim*; and that it was only beyond that limit that reigned the other *law*, of a decreaſing *moiſture*, correſpondent to the increaſing *maxima* of *evaporation* by an increaſing *heat*. That new *law* of *evaporation* offered evidently an adequate cauſe for explaining the anomalies obſerved in the *wet* veſſel; for *glaſs* retains concrete *water* very imperfectly, and it runs down very ſoon from many of the places which had retained it. Conſequently, under ſuch a partially *wet* veſſel, and differently ſo at different times, the inſtruments muſt be variouſly affected by ſcattered *atmoſpheres* of *extreme moiſture*.

80. However, before I could truſt that explanation, I wanted to ſubmit it to ſome direct experiment; and I ſucceeded by a means which at the ſame time realized what M. DE SAUSSURE thought he had obtained, namely, to produce *extreme moiſture* in a cloſe ſpace, during any common *temperature*,

without any *precipitation* of *water* from the *medium*. This I have produced by means of a wire cage, 4 inches in diameter, covered with cotton cloth, having at the top a *reſervoir*, by which the cloth is kept thoroughly *wet* for a long time; which cage beſides is incloſed in the glaſs jar inverted over water. In that apparatus, though in ſummer-time, every *hygrometer*, either *thread* or *ſlip*, moves and fixes itſelf, not ſo ſpeedily, but elſe exactly as if it was plunged into *water*, without any *ſuper-ſaturation* of the incloſed *medium*, or *precipitation* of *water* on the *hygroſcopic* ſubſtance.

81. We may ſee now that the idea of two ſorts of *extreme moiſture* is without any foundation. In order to enforce the neceſſity of taking the point of *extreme moiſture* in the *air* brought to that ſtate, and not in *water*, M. DE SAUSSURE ſays, "That the *hygrometer* is not to meaſure the *moiſture* of *water*, "but that of the *air*." This at firſt appears plauſible; however, in reality, *moiſture* is no more to be conſidered in *water* itſelf, than *heat* in the fluid called *fire*. *Water* is the cauſe of *moiſture*, as *fire* is the cauſe of *heat*; but thoſe *effects* are not produced *on* their cauſes; it is *on* other ſubſtances. Therefore, if ſome *hygroſcopic* ſubſtances are placed in a *medium* which has attained *extreme moiſture*, and, in proportion as they take *water* from it, the loſs of that *water* be conſtantly repaired by a new *evaporation*, they will receive by degrees in ſuch a *medium*, without any *precipitation*, as much *water* as if they were plunged in *water* itſelf; for the limit is their *capacity*, which I have explained in § 19. This is the ſame theory that I had expreſſed in my firſt Paper on *Hygrometry*; and it is completely confirmed by the above experiment, with the diſcovery of this new *hygroſcopic law*: "That in a *ſtagnant air*, every "*evaporating* ſurface has an *atmoſphere* of *extreme moiſture*, "which

" which extends in a ſpace of few inches, diminiſhes rapidly, " and does not interfere beyond that limit with the other *laws* " of *moiſture*."

82. I ſhall now explain, by an example, what is the fundamental *deviation* of an *hair hygrometer*, introducing in it, for a moment, thoſe two ſingular points 98 and 100, which, in M. DE SAUSSURE's experiments, were a very natural cauſe of miſtake. A *hair* hygrometer and mine, being in a cloſe veſſel, at a time when the *temperature*, ſenſibly conſtant, ſhall be but little above 32; if *moiſture* is firſt introduced into that veſſel, ſo as to bring the *hair* hygrometer, by a very ſlow direct motion, to 98, my *hygrometer* will ſtop between 70 and 75; and both inſtruments will be *fixed*, if *moiſture* and *heat* remain the ſame. Let *moiſture* then be made to increaſe very ſlowly, till the *hair* hygrometer has attained its point 100; mine will have arrived at 80; and they again will remain at thoſe points as long as, with the ſame *temperature*, the ſame quantity of *vapour* ſhall remain in the veſſel. Laſtly, let a ſufficient or ſuperfluous quantity of *water* be introduced into the veſſel, the *hair* hygrometer will *retrograde* to 98, and mine proceed to 100, at which points they will ſtop, whatever be the quantity of *water*; and they will remain fixed, as long as the *heat* ſhall not increaſe. This explains the riddle of the ſingular point 98, or of a certain *point*, various in different *hair* hygrometers, various even, at different times, in the ſame individual, at which that inſtrument ſtands with very different degrees of *moiſture*; conſequently, its little motions round that *point* may create great deception, eſpecially from the nature of *organized* ſubſtances, on which now I ſhall add a few words.

83. The above is the fundamental *march* of the *hair* hygrometer, ſuch as it is on the whole, and as it would be con-

ſtantly, if no other cauſe interfered; but it is ſubject to diſturbing *anomalies*, which become worthy of attention within that ſmall critical ſpace which I have deſcribed. The *texture* of organized *hygroſcopic* ſubſtances occaſions a *friction* between their parts, when, by the changes of *moiſture* and *heat* combined with their *elaſticity*, they undergo changes in their reſpective poſitions; whereby they hardly can completely return to the very ſame arrangement, though with the ſame external circumſtances; even at *extreme moiſture*, when, there being leſs *friction* between their particles, the greateſt part of the diſturbances produced in their former motions are reſtored. To that general cauſe of irregularity is added a particular cauſe, when thoſe ſubſtances are in the ſtate of *hygrometers*; this is the influence of two oppoſite forces acting conſtantly on them; one, the tendency of their component parts to remain united; the other, a *weight* or *ſpring* which tends to ſeparate them. Certain accidental arrangements of their component parts give them more power to reſiſt the action which tends to ſeparate them; and thoſe arrangements are very changeable, by the alternate introduction and expulſion of *moiſture*, by a long ſtay within a ſmall compaſs of variations, and by more or leſs *heat*. This is a large field of facts and ſpeculations, not unintereſting in itſelf, but on which I muſt not dwell: what I have ſaid of thoſe cauſes is ſufficient to account for the *anomalies* to which, more or leſs, every *hygrometer* is ſubject. But whereas in the *ſlips*, thoſe *anomalies* create only ſome irregularities in the obſervations, without any deceiving conſequence in reſpect of the *laws* of *moiſture*, they may deceive when they happen to interfere in the critical part of the march of ſome *threads*; for inſtance, if, by a certain accidental arrangement of the conſtituent parts in a *ſlip* of *whalebone*, there happens to be ſome

tenths of a *degree* difference, from one experiment to another, on its point of *extreme moiſture*, that *anomaly* cannot be of any conſequence on the determination of what muſt be conſidered as that ſtate in the medium; but if it happens to the *hair*, which, in approaching *extreme moiſture*, has but very ſmall motions, it may reverſe thoſe which it had had naturally (as I have obſerved it ſometimes) and become a cauſe of deception.

84. I have now explained, how mere *accidental* circumſtances have been the cauſe of a difference in the ideas that M. DE SAUSSURE and I had formed on what is to be underſtood by *extreme moiſture* in every caſe; and I am going to illuſtrate the whole of that ſubject by a ſingular fact. An *hygrometer* made with a *box thread*, or a thin *faſcicle* of the fibres of that wood, being placed in open air, next to a *hair* hygrometer, or to moſt of the other inſtruments of that ſort, moves in a contrary way from them; but we may lay aſide that circumſtance, by ſuppoſing, that the *numbers*, marked on the *dial* of the firſt, are increaſing in the oppoſite direction from thoſe of the other inſtruments. Let us then ſuppoſe, that ſome experimental philoſopher had choſen the *box thread* for his *hygrometer*; with him I ſhould have fallen into no controverſy on the point of *extreme moiſture*; for, either under the *moiſt veſſel*, or in any other caſe approaching *extreme moiſture*, his *hygrometer* would have moved like mine. But the *box thread*, at approaching *extreme dryneſs*, firſt relents much its pace, then becomes *ſtationary*, and afterwards *retrograde*; by which property, with the concourſe of ſome *accidental* circumſtances as have happened in M. DE SAUSSURE's experiments, the very ſame queſtions that I have examined with ſo much labour in reſpect of *extreme moiſture*, only becauſe of the *hair* hygrometer, would have been tranſported to the point of *extreme*

treme dryneſs; which, however, till now, has created no doubt.

85. That ſingular *thread* illuſtrates alſo another point, cloſely connected with the *retrogradation* of its tribe, but not to be confounded with it; I mean the *recoil*. The common cauſe of both phænomena is, two oppoſite *effects* produced by changes of *moiſture*, on the length of *threads*. The *retrogradation* in the *march*, comparatively to that of *moiſture*, is produced by one of the *effects*, which before was ſurpaſſed by the other, becoming predominant; and the *recoil*, or a returning back a part of a firſt ſtride when *moiſture* changes ſuddenly, is produced, by one of the *effects*, that on the *fibres* themſelves, being performed *ſooner* than that on the ſort of *reticle* formed by the *fibres*. Now, the *box thread* having its *retrogradation* at approaching *extreme dryneſs*, there alſo the *recoil* becomes ſenſible: it appears in the firſt modifications of that *thread* when placed in my *dry veſſel*, by motions backwards and forwards, as it happens to ſome other *threads*, when taken out of *water*, or expoſed to any other ſudden change of *moiſture*. The *box thread* having a ſlow motion, no *recoil* is clearly diſtinguiſhable in its common *march*; whereas the *hair* and the *quill-thread*, which, in appearance, are very quick, have generally a very diſturbed motion when *moiſture* changes ſuddenly. I have ſeen them, when in a free air, and happening to be in their *ſtationary* ſtate, moving quickly one way in a ſpace of 1 or 2 degrees, and then *recoiling* ſlower, ſometimes to the ſame point where they were before, while my *hygrometer* underwent a ſteady change, which was in the firſt direction of theirs. This phænomenon, of a complete *recoil* in the *ſtationary* ſtate of quick *threads*, is ſimilar to the *recoil* of the *index* in thoſe *glaſs frames* that I have deſcribed in § 59. of this Paper, which have a

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compenſation for the changes of *heat* by a thin *braſs lamina*: this being ſooner affected than the *glaſs rods*, by ſudden changes of *heat*, the *index* moves firſt one way; then it *recoils* completely, by the change being operated later in the *glaſs rods*.

86. The experiments briefly related in this Paper will, I hope, be ſufficient to anſwer the following queſtion, which has been made to me by ſome obſervers of the two principal *hygrometers* here compared: "Why does the *hair* hygrometer, "when expoſed to the open air in day time, come ſo often "near its point of *extreme moiſture*, while the *whalebone* hygro- "meter almoſt never comes within 30 *degrees* diſtance of that "point in ſummer, and very ſeldom within 20 in winter, even "in *rainy* weather (if preſerved from rain)?" The anſwer, according to the reſults of theſe experiments, is this: "The "general *march* of the *hair* hygrometer is much *decreaſing*, "comparatively to equal increaſes of *moiſture*; that *march* for- "wards, ends in a *ſtationary* ſtate, and is followed by a ſmall "*retrogradation*; while the *whalebone* hygrometer has con- "ſtantly a *march*, if not *proportional*, at leaſt conſtantly *ſimilar* "to that of *moiſture* itſelf."

87. There remains an object of inquiry, which is, a determination of thoſe *ratios* here generally expreſſed. I have explained, in the firſt *part* of this Paper, the difficulties of that object, and what help may be found in comparing the *marches* of *hygrometers* with the *acquiſitions* of *weight* of their ſubſtances, of which proceſs I gave ſome *examples*; and here I ſhall relate ſimilar experiments on *hair*, *whalebone*, *box*, and *aloes-pitta*. But as I have already deſcribed the whole proceſs, and the manner of calculation of that claſs of experiments, I ſhall only give here the reſults of theſe laſt.

88. I

88. I muſt, however, firſt explain another *reduction* that I have added to the former. In the firſt TABLE which I have given of thoſe experiments, I followed the immediate diviſion of my inſtruments, in which 0 correſponds to *extreme dryneſs*, and 100 to *extreme moiſture*. But firſt, under that form, the point called 100 by M. DE SAUSSURE would not appear in its true light, as it means the greateſt *length* of the *hair*; while the point 100 of my ſcale deſigns the ſtate of that *thread* in *water*, where it has a *little retrogradation*. Under that form alſo moſt part of the terms in the obſervation on the *box thread* would be *negative*, ſince it moves very long in a contrary direction to the other hygroſcopes. For theſe reaſons, inſtead of calling 0 the point of *extreme dryneſs*, and that of *extreme moiſture* 100, I have, in the following TABLES, applied the firſt of thoſe denominations to the *ſmalleſt length* of each ſubſtance, and the laſt to their *greateſt length*. That *reduction* produces no difference in the *proportions* between the *terms*, and none in the *terms* themſelves in reſpect to *ſlips*; as in theſe, the *greateſt length* is always obſerved by *extreme moiſture*, and the *ſmalleſt* by *extreme dryneſs*.

II. TABLE

II. TABLE *of comparative changes in the* weight *and in the* length *of the ſame ſubſtances, by the ſame* increaſes of moiſture, *correſpondent to the march of the* ſlip of whalebone *from 5 to 5 of its degrees.*

	WHALEBONE.		HAIR.		ALOES-PITTA.	
	Increaſes of the weight in ſhavings.	March of the ſlip.	Increaſes of the weight in a maſs of hair.	March of the hair.	Increaſes of the weight in a maſs of pitta.	March of the thread of pitta.
Extr. dryneſs	0.0	0	0.0	0.0	0.0	0.0
	6.0	5	4.8	15.7	6.0	20.6
	11.8	10	8.8	29.0	11.8	35.1
	17.3	15	12.5	40.0	17.3	51.6
	22.2	20	15.9	50.4	22.2	57.6
	26.8	25	19.1	59.7	26.8	75.6
	31.2	30	22.2	67.5	31.2	71.9
	35.2	35	26.6	74.4	35.2	76.3
	39.7	40	29.0	79.3	39.7	83.0
	44.0	45	32.0	83.3	44.0	86.6
	48.1	50	35.0	88.0	48.1	93.6
	52.1	55	38.2	90.0	52.1	96.5
	57.1	60	43.3	92.8	57.1	94.7
	61.7	65	49.8	94.1	61.7	98.2
	66.3	70	55.3	95.4	66.3	100.0
	71.9	75	61.9	97.0	71.9	99.2
	77.6	80	68.7	100.0	77.6	98.2
	* 83.2	85	* 76.0	99.5	* 83.2	96.8
	* 88 8	90	* 84.0	99.2	* 88.8	94.1
	* 94.4	95	* 92.0	98.6	* 94.4	91.9
In *water*	*100.0	100	*100.0	97.7	*100.0	88.3

89. In the above, and in the following experiments, the operation of ſucceſſively introducing *moiſture* into the veſſel was ſtopped, when the *ſlip* of *whalebone* was at 80, as beyond that term the ſmalleſt difference in the *temperature* between the parts of the apparatus creates great anomalies; therefore the fol-

lowing *terms* in the three columns of *weights*, which are marked with an *, have only been added (as I have explained for the former *Table)* with the view of having a common *modulum* between the changes of *weight* and the *marches* of the other inſtruments. But the obſerved *terms* remain in their original *proportions*, and from theſe we may ſee, that the *march* of the *ſlip* of *whalebone* does not differ much from the ſucceſſive increaſe of *weight* in its own ſubſtance; and that when taken out of the apparatus, and immediately plunged into *water*, it proceeds in the ſame direction as before, till it has attained its fixed point, while the *hair* takes, comparatively with the increaſe of *weight* of its own ſubſtance, great ſtrides in the beginning of its *march*, and very ſmall ſteps in the latter part of it, before 100; and then *retrogrades* a little, when taken out of the apparatus, and plunged into *water*. We ſee beſides in that TABLE, that the *thread* of *aloes-pitta*, which at firſt takes ſtill greater ſtrides than the *hair*, has, after a longer *ſtationary* or undetermined ſtate, a determined beginning of *retrogradation* at the ſame time that its own ſubſtance continues to acquire *weight* in the apparatus, and continues that *retrograde* march when, being taken out of the veſſel, it is plunged into *water*.

90. The following TABLE will farther illuſtrate theſe characteriſtic differences of *ſlips* and *threads*, the aſcertaining of which was ſo eſſential to *hygrometry*.

III. TABLE

III. TABLE *of experiments on the comparative changes in the* weight *and the* length *of the ſame ſubſtance by* increaſe *of* moiſture.

	Slip of whalebone.	BOX. March of the ſlip.	BOX. Increaſes of the weight in ſhavings.	BOX. March of the thread.
Extr. dryneſs	0	0.0	0.0	72.8
	5	4.5	7.3	87.2
	10	9.5	12.8	93.2
	15	14.5	17.8	97.8
	20	20.0	22.6	100.0
	25	25.7	27.3	95.9
	30	31.5	31.8	92.7
	35	38.0	38.5	88.6
	40	45.5	44.5	79.9
	45	51.5	49.7	70.3
	50	56.5	54.8	63.9
	55	61.2	59.1	57 3
	60	65.7	63.1	51.0
	65	69.7	66.4	47.5
	70	73.7	69.6	40.9
	75	77.7	76.6	31.4
	80	81.5	80.0	21.7
	85	85.9	* 85.0	16.0
	90	90.5	* 90.0	10.4
	95	95.5	* 95.0	5.1
In *Water*	100	100.0	*100.0	0.0

We ſee in this TABLE the *ſlip* of *box* following, in its increaſes of *length*, the increaſe of *weight* in the *ſhavings* of the ſame *wood*, nearly in the ſame manner as the *ſlips* of *whalebone*, *quill*, and *deal*, follow thoſe of their own ſhavings; while the *thread* of *box*, after having gained ſome length by

decreaſing ſteps, begins ſoon to *ſhorten*, at the ſame time that its ſubſtance continues to imbibe *water*; being thus the *ſhorteſt*, when it cannot receive any more *water* in its pores. That exceſs of the *hygroſcopic* phænomenon of *threads* cannot but throw a full light on the nature of thoſe *hygroſcopes*.

91. I am now going to aſſemble, in two TABLES, the comparative *marches* of all the *threads*, and of all the *ſlips*, which I have hitherto ſubmitted to that regular courſe of experiments, laying aſide many more of each claſs, the *marches* of which I only know from common obſervations. The next TABLE ſhall contain the experiments on *threads*; in the number of which are two thin natural bodies, which in that reſpect are ſimilar to the *hair*; one, an animal ſubſtance, is a very thin *porcupine quill*; the other, a vegetable, is a thin ſtem of *gramen*.

TABLE

TABLE *of the correſpondent* marches, *by the ſame* increaſes of moiſture, *of different* THREADS *or vegetable and animal ſubſtances taken* lengthwiſe.

	Porcupine quill.	Whale-bone.	Hair.	Gut.	Aloes-pitta.	Gooſe-quill.	Deal.	Gramen.	Box.	Slip of wh.bone.
Ext. dryneſs	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0	72.8	0
	18.0	12.0	15.6	9.7	20.6	37.0	33.2	26.8	87.4	5
	34.0	29.9	29.4	19.2	35.1	66.6	54.8	48.4	93.2	10
	48.8	39.9	40.9	26.8	51.6	78.7	† 74.9	67.1	97.8	15
	62.3	50.8	50.5	37.0	57.6	88.0	84.6	† 76.6	*100.0	20
	73.3	58.8	59.2	47.1	75.6	† 93.4	89.8	83.9	95.9	25
	81.0	65.3	68.8	57.3	71.9	97.2	93.8	90.5	92.7	30
	86.8	70.8	73.0	67.4	76.3	99.0	96.9	95.1	88.6	35
	90.8	76.1	78.3	75.6	83.0	94.4	94.3	98.6	79.9	40
	93.0	81.4	82.1	82.9	† 86.6	96.2	97.7	*100.0	† 70.3	45
	95.0	85.4	86.1	87.8	93.6	99.0	*100.0	98.8	63.9	50
	94.5	88.4	88.8	† 91.6	96.5	95.3	94.6	98.0	57.3	55
	97.0	90.8	91.6	94.7	94.7	97.2	97.0	97.2	51.0	60
	96.5	92.8	93.8	96.3	98.2	98.2	94.6	96.2	45.7	65
	96.5	95.1	95.6	97.8	*100.0	*100.0	93.0	94.8	40.9	70
	95.0	97.1	97.2	98.7	99.2	99.0	91.4	92.6	31.4	75
	97.0	98.1	† 98.0	*100.0	98.2	98.2	89.0	89.8	21.7	80
	98.0	99.1	100.0	98.7	96.8	97.2	86.9	86.5	16.0	85
	98.6	† 99.6	*100.0	96.8	94.1	95.8	84.6	84.0	10.4	90
	99.1	*100.0	99.3	94.5	91.5	94.4	81.9	80.9	5.1	95
In *water*	100.0	99.5	98.3	91.8	88.3	92.5	79.0	77.0	0.0	100

92. There

92. There the *porcupine quill* ſhews no *retrogradation*; however, conſiſtent with its tribe, it had ſome in other experiments. Its laſt ſteps have the unſteadineſs of the *ſtationary* ſtate, and thereby are ſubject to anomalies. From the ſame cauſe, none of the other *threads* have exactly the ſame ſteps in any two experiments, though on the whole their *march* remains eſſentially the ſame. The *march* here given of the *hair* hygrometer comparatively with mine, is the mean reſult of three experiments, with three different ſets of inſtruments; one of the *hair* hygrometers that I have employed was ſent to me by Mr. PAUL, of Geneva, and its point of *extreme moiſture* was determined in a *fog*. The ſmall and changeable *retrogradation* of the *thread* of *whalebone* and of *hair* might have been overlooked, were it not for other *threads* in which the *retrogradation* begins before that period where the ſtate of *moiſture* is difficult to aſcertain; but from theſe *threads*, that phænomenon is placed in a clear light, which is reflected on the others. I have marked with an * the greateſt *elongation* of each of them, and with a † a point near which their *elongation* begins, and to which they return at laſt. Theſe ſigns will guide the eye in the above TABLE, which ſhews clearly, that no *thread* can be truſted to for the HYGROMETER.

TABLE

TABLE *of the correſpondent* marches *of* SLIPS, *or of fibrous vegetable and animal ſubſtances taken acroſs the fibres, and of ſuch as have no ſenſible fibres.*

	Gooſe quill.	Porcupine quill.	Slip of wh.bone.	Box.	Deal.	Ivory. breadthwiſe.	Ivory lengthwiſe.	Tortoiſe-ſhell.	Horn breadthwiſe.	Horn lengthwiſe.
Ext. dryneſs	0.0	0.0	0	0.0	0.0	0.0	0.0	0.0	0.0	0.0
	4.8	4.8	5	4.5	5.4	6.1	8.3	11.0	9.5	13.8
	9.7	8.8	10	9.5	11.2	12.7	16.6	21.5	18.5	26.8
	14.4	12.0	15	14.5	16.5	18.7	24.6	31.5	27.5	38.8
	19.2	17.0	20	20.0	21.9	24.9	31.5	38.5	37.0	48.8
	23.9	23.4	25	25.7	27.2	30.4	37.6	45.4	46.5	58.0
	28.5	29.4	30	31.5	32.7	35.4	43,6	51.9	54.5	64.6
	33.3	36.0	35	38,0	38.3	41.9	49.7	58.3	62.1	71.0
	38.3	41.4	40	45.5	43.7	47.4	56.3	63.8	68.7	75.5
	42.9	45.4	45	51.4	49.2	53.5	62.4	69.0	72.3	78 5
	47.4	49.8	50	56.5	54.6	58.5	67.4	72.8	77.0	82.2
	52.4	54.8	55	61.2	59.9	63.5	71.6	76.4	79.7	86.2
	56.9	59.7	60	65.7	64.9	68.0	76.1	79.4	84.3	89.6
	61.9	64.4	65	69.7	69.7	72.1	79.1	82.4	86.4	92.4
	67.2	68.5	70	73.7	74.5	76.1	82.9	84.9	88.4	93.4
	72.2	73.5	75	77.7	79.0	80.1	86.7	88.2	90.2	94.4
	77.8	78.9	80	81.5	83.5	84.5	90.4	91.2	92.0	95.4
	82.8	83.9	85	85.9	87.5	87.8	92.4	93.8	94.0	96.6
	88.2	88.9	90	90.5	92.0	92.0	94.5	96.2	96.1	97.8
	94.0	94.4	95	95.3	96.0	96.0	97.5	98.6	98.1	99.0
In *water*	100.0	100.0	100	100.0	100.0	100.0	100.0	100.0	100.0	100.0

93. This.

93. This laſt TABLE is the moſt important, as it contains a claſs of *hygroſcopes* which poſſeſs in common the following firſt requiſites for an *hygrometer*; 1ſt, that they may indicate, without any illuſion, both *extreme dryneſs* and *extreme moiſture*; 2dly, that they move conſtantly in the ſame direction as *moiſture* itſelf; 3dly, that they move always when *moiſture* changes. It ſhould ſeem as if the *march* of the *ſlip* of *horn* taken *lengthwiſe*, from its very decreaſing progreſſion, came very near that of the thin *porcupine quill*; but, as I have ſaid, among the ſteps of the latter there are accidental *retrogradations*, and it ſometimes has a final one; and I have never obſerved that diſpoſition in the former, which, in its laſt ſmall ſteps, follows conſtantly the motions of every other *ſlip*.

94. The agreement of all the *ſlips* in this laſt reſpect is a very eſſential circumſtance in hygrometry, as it aſſures us, that we cannot miſtake the caſes when *moiſture* is *extreme* in the *atmoſphere*; a very important point for diſcovering the nature of many *meteorological* phænomena. No *ſlip* will create deception in that reſpect; while, on the contrary, every *thread* may deceive in dubious caſes, and even create great error, if, unknown to the obſerver, it happened to be in the beginning of its *elongation*. There was, however, a queſtion to be decided in that reſpect, namely, whether or not a great *moiſture* in the *medium* was a cauſe of alteration in the *march* of any *hygroſcope*, by producing in its ſubſtance a ſudden irregular *lengthening*. That accidental queſtion is anſwered in the negative by all the *hygroſcopes* of both claſſes: for, in reſpect of the *threads*, inſtead of *lengthening* ſuddenly in that period of *moiſture*, they have then a *retrograde* motion, either continuing or only beginning; and as for the *ſlips*, they, by *lengthening* in the ſame period, only follow their former *laws*: the *ſlips*

which, comparatively to that of *whalebone*, have at firſt ſmall *ſteps*, and which conſequently move in an increaſing progreſſion, continue only to follow that progreſſion; and thoſe which at firſt have greater *ſteps*, and conſequently a decreaſing march, have then ſmall *ſteps* conformable to their individual *law*; therefore, none of thoſe *hygroſcopes* of both claſſes have any *ſudden ſtart*, produced by any degree of *moiſture* in the *medium*, or by the application of concrete *water*; each of them follows, from one end to the other of its ſcale, it own *progreſſion*; and in reſpect of *ſlips*, moiſture is never *extreme* in the ambient *medium*, as long as, in their reſpective *progreſſions*, they have not attained their *greateſt length*.

95. Our common *hygrometer* muſt then be made of one of the *ſlips*; but with that great diſſimilarity obſerved in their *marches*, which of them ſhall we chooſe as indicating the real *march* of *moiſture*? None as yet from that conſideration, which I do not even think a primary one. It is true, that if we truſt to the increaſes of *weight* in thoſe ſubſtances, as being a means of aſcertaining the real progreſs of *moiſture* in the ambient *medium*, the mean rate of ſix experiments of that kind related in this Paper, give the preference to the *ſlip* of *whalebone*; but this I do not yet conſider as deciſive, farther than in what relates to the comparative *marches* of *ſlips* and *threads*; however, as my reaſons of doubt on a more abſolute concluſion cannot be expreſſed in a tranſitory manner, I muſt lay them aſide for the preſent.

96. But, as I have ſaid before, this is not what ought to determine our choice on the *ſubſtance* of a common *hygrometer*, ſince the obſervations themſelves are diſtinct from the conſequences to be drawn from them. Let us ſuppoſe the caſe (which I do not give up) that, with time and reſearches, ſome

proceſs be found by which *known* quantities of *moiſture* may be ſucceſſively produced in the *medium* itſelf. The uſe of that proceſs for hygrometry will be, as M. DE SAUSSURE has begun to do it, to obſerve, on ſome *hygrometer*, the ſucceſſive effects of thoſe known quantities of *moiſture*, from which may be formed a *table* of the correſpondence between the *equal degrees* of the ſcale of the choſen inſtrument, and the real quantities of *moiſture* in the *medium*; and that *table* will ſerve to correct as well *paſt* as *future* obſervations made with that inſtrument. Therefore it matters not what that *hygrometer* ſhall be, provided it is convenient in other reſpects. Let us then examine which of the *ſlips* poſſeſſes the moſt eſſential properties of an *hygrometer*, ſuch as ſhould be in common uſe for comparative obſervations, and to which conſequently future diſcoveries in reſpect of the real proportions between the quantities of *moiſture* itſelf would be applied.

97. *Steadineſs* is ſurely a firſt requiſite for ſuch an inſtrument; and in that reſpect no *ſlip* comes in competition with that of *whalebone*. That property was the firſt motive of my choice; and as an inſtance of it I ſhall only mention, that I have juſt now plunged into *water* an inſtrument of that ſort, of above ten years ſtanding, which is come to its point of *extreme moiſture* as if it had been fixed yeſterday; for, without regard to the diſtance of obſervations, there may be between them a difference of ſome *tenths* of a *degree*. Some other *ſlips* may be brought to a certain degree of *ſteadineſs* by ſtudying what is the degree of *ſtretch* which they may bear; but that attention is not neceſſary for the *ſlip* of *whalebone*: if, for inſtance, when its point of *extreme moiſture* has been fixed while it was *ſtretched* to a certain degree, that *ſtretch* is

much increaſed, it will acquire ſome abſolute *length*; but it will be *ſteady* again for a new point taken then in *water*.

98. Another property of the *ſlip* of *whalebone*, which at firſt ſhould ſeem contradictory to the former, is its great *expanſibility*, in which alſo it ſurpaſſes all the ſubſtances which I have tried. Such a *ſlip* lengthens above *one-eighth* of itſelf from *extreme dryneſs* to *extreme moiſture*, which produces many advantages in the conſtruction and obſervation of that inſtrument. In reſpect to obſervation, when it is expoſed to the wind, the difference between the chords of the arches of its bends and its real *length* is ſo ſmall, comparatively with its *hygroſcopic* variations, that the indetermination of its *index* will remain confined in a ſpace of one or two *degrees*, when it becomes impoſſible to obſerve *hygrometers* whoſe ſubſtance has but little *expanſion*. Laſtly, of all the ſubſtances which I have reduced to *ſlips*, none is ſo eaſily made thin and narrow as *whalebone*. I have found means for producing eaſily ſuch *ſlips* of it as, with a length of eight *inches*, weigh only about $\frac{1}{10}$th of a *grain*, and are thereby as *quick* as is convenient in other reſpects. All thoſe diſtinctive properties of the *ſlip* of *whalebone* ſeem to point out an *hygroſcopic* ſubſtance fit for our *common hygrometer*.

Deſcription of the whalebone hygrometer.

99. I have now only to deſcribe the conſtruction of that inſtrument as I have fixed it after a long experience. The *fig.* 1. (Tab. IX.) ſhews its form for common uſe. Some of thoſe inſtruments are of the ſame ſize as the *figure*, and they may eaſily be made ſmaller, but commonly they are half as large again in every dimenſion. Their frame will ſufficiently be known from the *figure*,

therefore I ſhall confine myſelf to the deſcription of ſome particulars. The *ſlip* of *whalebone* is repreſented by *a*, *b*; and at its end *a* is ſeen a ſort of *pincers*, made only of a flattened bent wire, tapering in the part that holds the *ſlip*, and preſſed by a ſliding ring. The end *b* is fixed to a moveable bar *c*, which is moved by a ſcrew for adjuſting at firſt the *index*. The end *a* of the *ſlip* is hooked to a thin braſs wire; to the other end of which is alſo hooked a very thin ſilver gilt *lamina*, that has at that end *pincers* ſimilar to thoſe of the *ſlip*, and which is fixed by the other end to the *axis* by a pin in a proper hole. The *ſpring d*, by which the *ſlip* is ſtretched, is made of ſilver gilt wire; it acts on the *ſlip* as a *weight* of about 12 grains, and with this advantage over a *weight* (beſides the avoiding ſome other inconveniencies of this) that, in proportion as the *ſlip* is weakened, in its lengthening by the penetration of moiſture, the *ſpring*, by unbending at the ſame time, loſes a part of its power. The *axis* has very ſmall *pivots*, the *ſhoulders* of which are prevented from coming againſt the frame, by their ends being confined, though freely, between the flat bearing of the heads of two *ſcrews*, the front one of which is ſeen near *f*. The ſection of that *axis*, of the ſize that belongs to a *ſlip* of about 8 inches, is repreſented in *fig.* 2.; the *ſlip* acts on the diameter *a*, *a*, and the *ſpring* on the ſmaller diameter *b*, *b*.

100. Another conſtruction of the ſame inſtrument is repreſented, of half its dimenſions, by *fig.* 3. (Tab. X.) The eſſential parts of that *hygrometer* are the ſame as in the other; therefore I ſhall only mention the differences adapted to the uſe of fixing it out of a window. The *figure* repreſents the manner in which it is fixed, with its *dial* turned half-way towards the obſerver, by a proper head of the hooks which hold it at the top and the bottom.

bottom. The ſteady part of the inſtrument is repreſented by *fig.* 4.; it is partly an half tube, cut longitudinally through the axis, and having ſidewiſe two vertical rows of large holes. A whole tube goes over that half one; for which purpoſe the piece *a*, *a*, is taken off by unſcrewing the female ſcrew *b* which belongs to the part *d*; this laſt is a ſmall open tube, cut outſide in ſcrew. The large external tube has alſo two vertical rows of holes, at ſuch a diſtance from one another, that, when one of them correſponds to one of the rows of the half tube, the other is in front. The two oppoſite poſitions that tube can be brought to are in order that the inſtrument may be placed either ſide of a window, and each of thoſe poſitions is determined by one end of a cut at the bottom of the tube in *a*, *fig.* 3. which then holds againſt the ſteady pin *c*, *fig.* 4. The rows of holes of the tube are to be turned towards the room, to prevent the rain from falling on the *ſlip*; and the *dial* being incloſed in a box with a glaſs in front, no rain can get into the inſtrument. It muſt alſo be fixed in a place not much expoſed to the ſun, or be ſcreened from it without preventing the circulation of the air. The communication of the *ſlip* with the external air through the rows of holes and the open bottom *d*, *fig.* 4. is ſufficient for that claſs of obſervations. By the manner of its being hooked, it may be eaſily taken off for carrying elſewhere; and, if a quick obſervation is wanted, the tube alſo may eaſily be taken off.

I have the honour of preſenting one of thoſe inſtruments to the Royal Society; and, as it is very deſirable that ſome hygrometer be added to the other meteorological inſtruments uſually obſerved, I wiſh this may deſerve a place in their Obſervatory for that purpoſe.

Fig. 1.

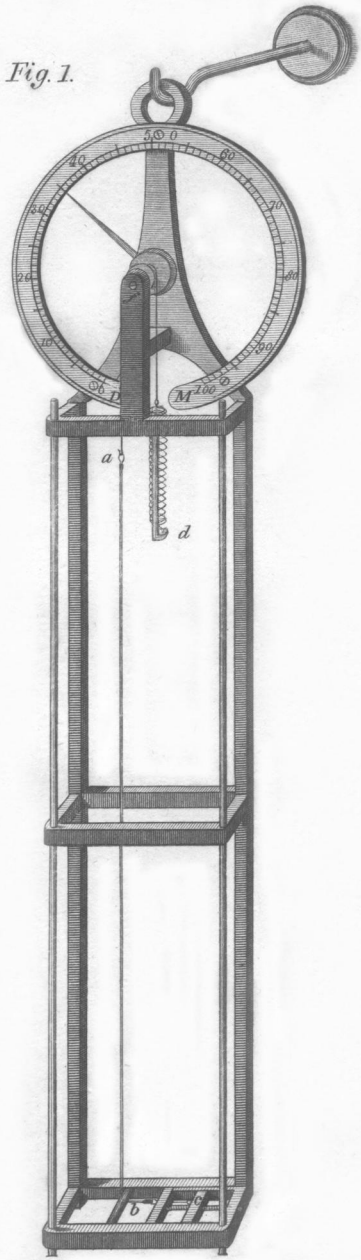


Fig. 2.

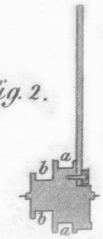
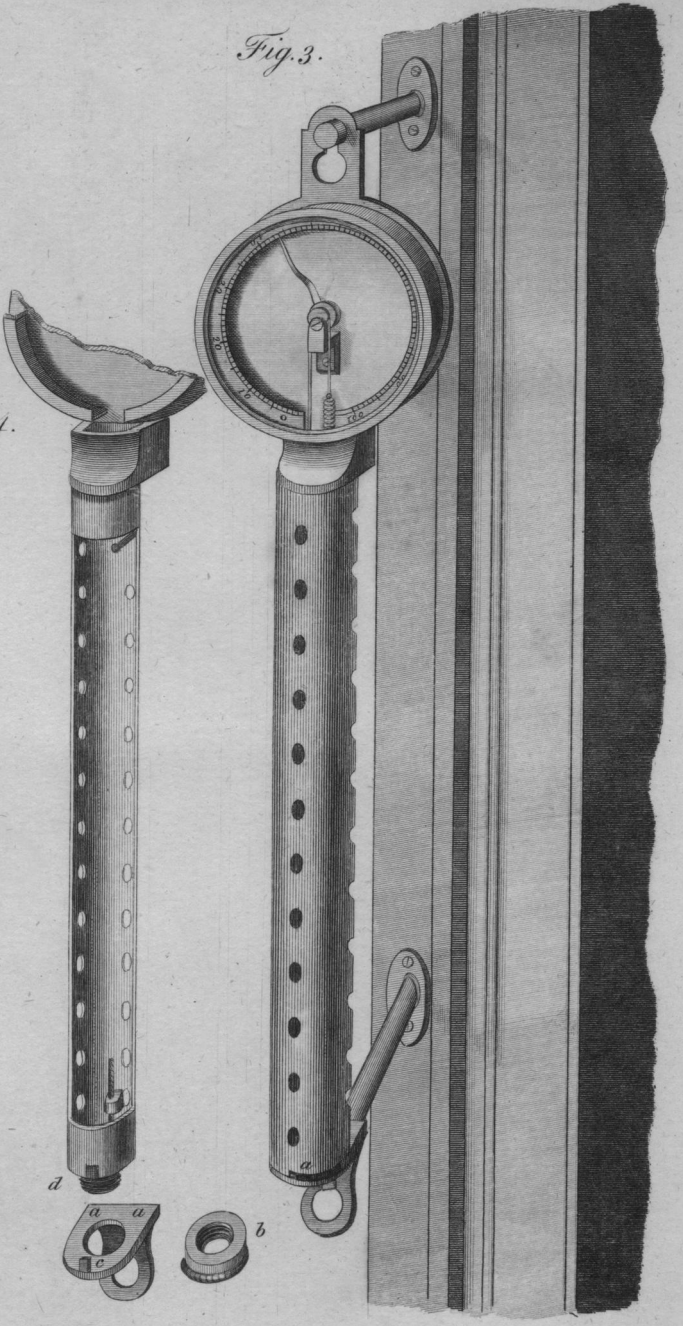


Fig.3.

Fig. 4.



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